

5726

And. Ross

E S S A Y S

ON

PHYSIOLOGICAL

S U B J E C T S,

K

By J. ELLIOT, Apothecary.

L O N D O N:

Printed for J. JOHNSON, No. 72, St. Paul's Church Yard.

1780.

PRICE ONE SHILLING AND SIX-PENCE.

ESSAYS

OF

PHYSIOLOGY

AND



Library

LONDON

Printed by J. G. Smith, 10, Pall Mall

1801

Printed and Sold by J. G. Smith, 10, Pall Mall

P R E F A C E.

SINCE publishing my last pamphlet † I have been favoured with an anonymous letter signed a Friend to active Merit, which has occasioned my venturing the following pages into the world. I embrace this opportunity of returning my most grateful thanks to the author for the judicious and friendly advice which it contains, and also to request that he would add to the obligation by favouring me with his address.

Of the public however I must beg pardon for appearing again so soon in print. It may with reason, be concluded, from the past, that I am like-

† An Address to the Public, &c.

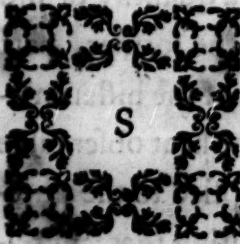
ly to be very troublesome in future. My apology is, that it has been justly represented to me that I acted improperly in subjoining a Philosophical Essay, to my Address to the Public, and was advised to separate them. The remainder of the subject has occurred to me since. And my reason for giving that also is that I might have the opinion of better, and less partial judges than myself, with respect to its probability, before I make any farther use of it.

It would be the height of ingratitude not to return my sincere and cordial acknowledgments for the great candour, or rather, lenity, and partiality which have been shewn me on account of my books, as well by the critics as others. Their good opinion it will ever be my highest ambition to deserve.

May 3, 1780.

E S S A Y S
ON
PHYSIOLOGICAL
SUBJECTS.

PART THE FIRST.

 **S**OME time ago the learned and ingenious Dr. Priestley discovered and demonstrated by a variety of experiments, that the use of respiration is “to carry off the phlogiston which the blood acquires during its circulation through the body.”

The

The principal facts on which this proposition is founded are, that the air which is expired by animals is phlogisticated ; that air is unfit for respiration, or supporting animal life, according as it is already combined with phlogiston ; that the arterial blood has a strong attraction for that principle, which is not the case with the venous. And that the florid colour of the arterial blood depends on the loss of phlogiston to the air during its passage through the lungs.

In a book which I have lately published,† there is submitted a theory of *combustion* ; by which it appears probable that "the phlogiston of the inflammable body is attracted from it by the air by means of a superior affinity, and that the heat depends on such combination of the *phlogiston* with the *air* ;" in other words, that "a quantity of fire is extricated from the latter by the former, part of which is transferred to the basis of the inflammable body, the remainder causing the heat observable in that process." This theory I applied to the phenomena of animal heat, by suggesting that "in respiration the phlogiston of the blood extricates,

† Philosophical Observations, &c.

N. B. The errors contained in that book I hope to be enabled, by a more extensive reading, to correct in a future edition.

in like manner, a quantity of fire from the air ; part (or rather, it seems, the whole) of which is absorbed by the blood," and that " the blood thus charged, and circulating through the body, has its fire extricated from it by the phlogiston which it imbibes in its passage from the arteries to the veins; part of which fire not being absorbed, is the cause of the heat of the body," The ingenious Dr. Crawford has illustrated this doctrine in a more convincing manner, in his excellent treatise on those subjects. He proves by experiments that the arterial blood contains more fire than the venous; inflammable bodies less than unflammable ones; and that air contains less of this principle, as it is more copiously combined with phlogiston.

Dr. Crawford contented himself with supposing that the phlogiston is furnished to the blood by the putrescent parts of the system. From the known fact, *that the heat of a part is less after the nerves which serve it are divided, than before.* I had inferred that " it is derived from the nerves," or at least that " the nerves are instrumental in such communication."† In an essay since published,
B
lished,

† The cold generated in the famous experiments in an heated room are easily accounted for from hence, and from what

lished, subjoined to *an Address to the Public, &c.*† I have shewn that “ ’tis on those nerves only, which serve for the *vital* or *involuntary* motions of the body that animal heat depends,” that heat being nearly as great in sleep as in waking, tho’ in the former case, the nerves serving for sensation, and voluntary motion, are inactive.

But I had endeavoured to shew that respiration conduces also to a far more general, and important end than the heat of the blood, nothing less than the *vital*, and *other motions of the body* depending on it. That for a nerve to cause the action of a fibre, it is necessary that the nerve should impart phlogiston either immediately, or mediately, to the blood flowing through or by that fibre ; or in other words, that “ there is a necessary connection or dependence between the action of the fibres, and the phlogistication of the blood.”

The truth of this doctrine appears I think by the following considerations. 1, The blood when

what follows. The heat of the room relaxing the fibres, their action, and in course the heat they generate, being less than usual ; at least this may be one cause, and the contrary of cold. The like may be observed of the heat in topical inflammations, fevers, &c.

† See page 20 hereof.

when it enters a muscle (the heart for example) by the artery, contains less phlogiston than when it comes out of it by the vein ; and that this difference proceeds from the cause assigned seems probable from hence, that if by inclosing an animal, in a proper vessel, the venal blood be suffered to return repeatedly into the muscle by the artery, the action thereof becomes weaker as the blood looseth its power of imbibing phlogiston, till at length it wholly ceases. Hence it seems to be chiefly that animals thus circumstanced die ; and hence also we have a reason why the motion of the blood through a muscle is necessary to its action. 2, It appears by Dr. Crawford's experiments that the heat of the blood ariseth on account of the phlogiston which it imbibes in the course of its circulation. But I have endeavoured to shew that it depends on the moving nerves, or on the action of the muscular fibres serving for the involuntary motions of the body ; and this being supposed, it follows that the action of these fibres depends on the cause assigned. And as the action of the voluntary and involuntary muscles doubtless depend on the same principle, the action of the muscular fibres, voluntary as well as involuntary, seems to be connected with the phlogification of the blood. †

It would seem from hence that the other functions of the body are subservient to the principle just advanced ; at least in part. The following considerations will perhaps render that opinion in some degree probable.

Observation 1. I lived for some time wholly on bread and raddishes, (the whole plant) and afterwards for a longer time on fat mutton and beef. In the latter case bile seemed to abound much more in the intestines and fæces than in the former ; and therefore is probably employed in greater quantity by vegetable, than by animal food. To be further satisfied in this matter, I digested in half of the gastric juice taken from a dog's stomach, some bread and raddish leaf, and in the other half some fat and lean mutton; then
added

‡ In syncope, when the vital parts act but weakly I have observed the venal blood to be more florid than usual.—The hearts of eels and the intestines of rabbits ceased their motions rather sooner in fixed and noxious, than in the open air, tho' the difference might have arisen from other causes than the non-dephlogistication of the blood.—Also it has been said that respiration carries off phlogiston from the putrescent parts of the system ; but is an healthy animal in any degree putrid ? In diseased states this may also perhaps be the case. I pretend not however to offer the foregoing proposition otherwise than as a conjecture.

added to each a little of the animal's gall. The proportions were alike in both cases ; but the animal mixture tasted strongly of the bile, the other scarcely at all ; and tho' I doubled the quantity of bile in the latter it still tasted less of it than the other, if I did not deceive myself.

Obs. 2. After the vegetable course I took a little blood from a vein in my arm, and I did the same after the animal ; it was lighter coloured in the former case than in the latter. The action of the fibres was also more slow and languid ; the heat of the body seemed less, and the temper, which is known to depend much on the state of the fibres, more calm. Now the *red* part of the blood is that which attracts phlogiston from the body, and parts with it to the air ; but there was a greater proportion of this when animal food was used, and therefore the blood's attractive force was greater. But by the foregoing theory the action of the muscular fibres will be greater when the blood attracts phlogiston more powerfully, and hence also the reasons of the different phænomena in the cases mentioned ; tho' *stimulus* may perhaps have some share in them.

Obs. 3. By comparing the two preceding observations, the use (at least one use) of the *bile* in

in the system will appear. Vegetable food is less disposed to form blood than animal, and therefore employs more bile. And it seems probable that the aliment, after solution in the gastric juice, according as it is disposed to form blood having less attraction for phlogiston, combines with a greater proportion of bile. Different kinds of animal and vegetable food however, may differ in the respects mentioned. Animal food may also *occasion* more bile than vegetable.

Obs. 4. When blood contains a greater proportion of *equally* colouring particles, its colour will be deeper than when their proportion is less. Hence the difference of *colour* of blood in the foregoing cases; with which its attractive force seems to be connected. I found also, agreeable to this, that by exposing the blood in those cases to the air, the latter did not become so florid as the former. The uses to which these observations, if well founded, may be applied, need not be pointed out to the philosophical physician. It may be remarked however that the proportion of red globules to that of the coagulable lymph in the crassamentum is variable, as well as that of the crassamentum to the serum. The colour of the blood therefore must either be noted before the coagulation, or else that effect must be prevented.

vented. By not attending to this circumstance I was at first misled.

Obs. 5. There are two principles of motion in the animal system, *elasticity* and *muscular power*. The latter seems to be influenced by the *red blood*.

Quære. Is not the former recruited by, or does it not derive its support, or nutriment from the *coagulable lymph*? If so, by observing its proportion in the blood, and its texture, the state of the *elastic power* in the body may also be conjectured. † I found that the vegetable diet was less disposed to form this part of the blood than the animal; and that it was less easily recruited, than red blood, by animal food. The *bile* therefore seems to be concerned in the production of this substance, as well as of the red globules. Supposing the above quære true, is not a fizy state of the blood an hindrance to the assimilation of the coagulable lymph into the elastic parts of the body? Certain phænomena of consumptions, &c. seem to shew this. And it may be the case with nutrition in general.

Obs. 6. That the bile affects the attractive force
of

† Whether those animals which have less *elastic power*, have not also less coagulable lymph? If I remember right, the blood of those fishes which we most commonly eat is thinner than that of sheep, oxen, or men.

of the blood in the manner mentioned seems probable from what happens in the jaundice. Bile in that case is deficient in the intestines, and is it not in consequence of this that the patient is heavy, sluggish, and languid? That bile does not act merely as a stimulus on the vascular system, as some have thought, is likewise probable from what happens in the same disorder; for tho' it exists naked in the vessels, the patient is less brisk and lively than usual. The healthy blood of the choleric constitution seems to be equally bland and soft with that of the phlegmatic. Also *choleric*, and *phlegmatic*, are used as synonymous terms to the different states of body and temper described in Obs. 2, whether these differences be constitutional or accidental.

Obs. 7. Physiologists have usually imagined that the bile, and pancreatic juice, have contrary uses in the system. As the use of the former seems to be to *encrease*, may not that of the latter be to *diminish* the attractive force of the blood? Has aliment which after solution in the salival and gastric juices, would form blood having too weak an attractive force, a power of combining with *bile*, rather than with *pancreatic juice*; and the contrary? Both these fluids however, may also have other uses than those; as the

the thickening, or attenuating of the chyle ; the addition of phlogiston to the aliment in cases where it is deficient, &c. &c.

I have only started these ideas by way of hint to be prosecuted by others, not having proper conveniencies for pursuing these enquiries myself, and therefore any inaccuracies or errors into which I may have fallen will be pardoned by the candid, especially as I do not mean to trouble the public in this way again. There are however other considerations to be attended to by the enquirer into these matters. Thus, the blood vessels may be too full, or too empty ; the red globules, according to the food, &c. may vary in their attractive power; and the blood will be more or less compleatly decomposed by the air : the colour both of the arterial, and venal ; the attractive force of the former, and the action of the fibres,* &c. in consequence thereof will be varied accordingly. The blood may be in a diseased state, as with respect to the *internal cause of blackness*,

* Do not the particles composing a fibre *repel* not *attract* phlogiston, and hence their encreased attraction, and the shortening of the fibre which they compose, in consequence of the nervous influence ? Is it not from hence that phlogiston is not derived into the fibre from the nerve but by *force*, as by irritation, or the will ? And hence the ease with which it is attracted from the fibres by the blood ? The known facility and quickness of muscular action is easily conceived on this idea, especially if the phlogiston be supposed to exist in the nerve in an uncombined state.

ness, &c. Thus also I think I have observed that it is more difficultly reddened by air when the coagulable lymph is very viscid. For all which, and perhaps other circumstances, as the greater or less irritability of the fibres, &c due allowance must be made. It may likewise be observed that in different animals the gastric juice is different, according to their food ; and that by attending to these and other particulars, as the proportion, and acrid state of their bile, the proportion of the elastic, to that of the muscular power, and the like, the reasons of their different natures may also probably be understood. But much time and attention would be necessary to this latter enquiry. What respects the attraction of the blood for phlogiston, seems to be more easy of investigation, and promises to be use. I cannot but think that Dr. Priestley's physiological discoveries have been too much neglected, and that much light might be obtained relative to the animal œconomy, and the practice of physic by prosecuting them. An instance of the latter I have submitted above in what relates to aliment, sanguification, and the phlogistic and antiphlogistic regimen. And with respect to the former it may be observed that from the conjecture which these discoveries suggested to me that "the action of the muscular fibres is connected with the phlogification of

of the blood," the explication of some of the most important phænomena of the system seem to follow. I may instance muscular motion; the final causes of respiration: the uses of the liver; and pancreas; the heat of the blood, and the reason, or necessity of its motion, and pulse; together with some other interesting particulars which the reader may have observed.† A theory is more probable as it is more general, and takes in a greater number of phænomena. The present one seems to comprehend the principal functions of the animal œconomy, and therefore may perhaps be considered as something more than a mere hypothesis. The leading proposition, if true, will perhaps appear to be the most important that has yet been discovered in Physiology.†

C 2

A

† See Philosophical Observations, Section 12. I have also applied these principles to other phænomena of the system.

† In my last essay I gave a conjecture concerning the particulars of the phlogification of the blood. But it appears from what has been said, that the *phlogiston itself* is the moving principle in the nerves. That it is derived from them into the fibres, either by irritation, or the will; (hence their contraction) and is attracted from these by the blood, which occasions their relaxation.

N. B. Fishes seem to breathe nothing but *water*, which is known to attract phlogiston.

*Extracts from the Pamphlet mentioned in
Page 10.*

The nerves may be divided into three classes, 1, those which serve for sensation. 2, Those which minister to voluntary motion. And 3, Those on which the vital or involuntary motions depend.

During the time of sleep, the heat of the body is as great, or very nearly so, as in waking; tho' the nerves serving for sensation, and voluntary motion, are then inactive. The like may be observed of an hysteric, or apoplectic fit, &c.† and yet the heat of the body also depends on the communicating of phlogiston to the blood, as I have endeavoured to make appear, and as the ingenious Dr. Crawford, has more fully shewn,

The heat of the blood therefore depends on those nerves, or on the action of those fibres which serve for the *vital* or *involuntary* motions of the body.

In

† N. B. There is a well known difference between these and faintings, &c.

In violent exercise however when the voluntary muscles act powerfully, the heat of the blood is encreased. The reason of which is, that its motion being quicken'd, its stimulus on, or action against the involuntary parts is greater ; so that more blood is phlogistified by those fibres than usual, their action being encreased, and therefore more heat is generated.

We have no reason for supposing but that the motions of the voluntary and involuntary muscles depend on the same principle ; and the like may be observed of the muscles of hot and cold animals.

But there is a great variety with respect to *heat*, which seems to proceed from the following causes.

Case 1, When phlogiston is communicated to the blood by means of nerves serving for *vital* or *involuntary* motion, the fire extricated from the blood is absorbed by the parts, furnishing the phlogiston *only in part*.

Case 2. When phlogiston is communicated to the blood by means of the nerves serving for *voluntary*

voluntary motion, the fire extricated is either *wholly, or very nearly* absorbed by the parts furnishing the phlogiston. They may even have a power of absorbing *more fire* than is extricated from the blood. But the former seems to be the truth.

Heat therefore must be produced in the *first* case. In the *second*, either *no heat, very little, or even cold*, may be occasioned according to circumstances, tho' probably neither heat nor cold.

In cold animals the *second* case may obtain both with respect to the *voluntary* and *involuntary* parts. Or it may obtain in the *voluntary parts only*, and in the *involuntary parts* the *first* case may take place, tho' in a less degree than in hot animals; and in different animals this may obtain in a greater or less degree; hence their various temperatures.†

These cases were suggested to me by the consideration that the cause of muscular motion must be the same in all the instances above specified, and the following experiments would be a test of the truth of the opinion.

Experiment

† Some animals are colder than the surrounding medium

Experiment 1. By comparing the absolute heats of the flesh of a *bird*, and of a *fish*, whether that of the fish would not appear to be the greatest ?

Experiment 2. By comparing the absolute heat of the flesh of an ox's heart, with that of the muscles of the leg, whether that of the heart would not appear to be the least ?

In the second experiment it must be remembered that the *heart* serves chiefly for *involuntary motion*. That its sensible heat, during life, is greater than that of the voluntary muscles (in hot animals) appears by its fat, which for this reason, is made to congeal sooner, or in a greater degree of heat, than that of other muscles ; and this is also an argument that the absolute heat of the heart is less than that of the voluntary muscles.

It must be remembered too that the muscles serving for *voluntary motion* are likewise continually exerting involuntary action.

Does the motion of the fibres of vegetables depend on the same principle as that of animals ?

P A R T T H E S E C O N D .

Observation 1st. My hand feels neither hot nor cold. If I put it into water of the same temperature, the neutrality continues. But if the water be of a greater degree of heat than my hand, it feels warm. If of a less degree, cold. And the sensations are stronger, as the difference of temperature is greater.

Water of the same temperature with that which is at present neutral, may at a future time feel warm ; and at another time, cold.

It may here be observed, 1st. That the hand itself in three different states of temperature, at different times, was neutral; and 2d. That when it was restored to the temperature in which it formerly was neutral; it felt in the one case heat, and in the other cold.

As at different times the hand, tho' at the same temperature was hot, cold, and neutral, does it not argue that the cause of these variations was not in the hand, but in the sensory, where these differences were perceived? Was not the state of the sensory different at those times, and hence the cause of the variations observed?

Obs. 2.

Obs. 2. My hand is now neutral. If I put it into water somewhat warmer than it, a sensation of heat is at first perceived. But after a while, the water, tho' it continues of the same warmth, becomes, by degrees, perfectly neutral. The like may be observed with regard to cold.

If after either of these cases, the hand, brought to a state of neutrality with respect to the water, be put into water of the same degree of heat with the hand before the experiments, tho' these degrees of heat would before have been neutral, yet in the former case cold, and in the latter, heat will be felt; and yet by continuing the hand awhile in the water, it will again become neutral.

Does not the sensory in these cases seem to have altered its state? Was it not different at the end of the first experiment from what it was at the beginning? And at the beginning of the second experiment from what it was at the end? and was it not varied to and fro several times in the course of the third? And does not the sensory seem to have a power or faculty of accommodating its state to that of the hand, so as to bring the sensations of heat or cold to a state of neutrality?

Obs. 3.

D 2

Obs. 3. When the difference of temperature between the water and hand is greater than in the preceding cases, it will be a longer time before the sensations be brought to neutrality. And if the difference be sufficiently great, they cannot be rendered neutral.

When therefore the difference is greater, is there not a greater difficulty in the sensory of adapting itself to the heat or cold of the hand ? And when the difference is excessive, does it not seem probable that the sensory cannot wholly accommodate itself thereto ?

Obs. 4. Water of the same temperature will be hot, cold, and neutral, to different parts of the body at the same time ; or in other words, different parts of the body may, at the same time, be of different degrees of heat, and yet all of them be perfectly neutral. And the phænomena may be varied in each particular part in the same manner as hath been shewn with regard to the hand.

May not the portions of the sensory which answer to the different parts of the body, therefore, be in different states at the same time ? May they not be varied with respect to each other ? and may they not all be brought to the same state ?

Obs. 5.

Obs. 5. Two different degrees of heat cannot be rendered neutral in parts of the body which are near to each other so soon, or so well, as in parts that are more remote. And degrees which may be rendered neutral in parts which are distant, cannot be made so in parts that are near ; if my observations be just.

Are not parts of the sensory therefore which correspond with contiguous parts of the body, also contiguous ? For does it not seem to appear that they influence each others states ? which the portions of the sensory answering to distant parts of the body, do not, or not in so great a degree ? Does not the present observation seem to point out that the state of the sensory discoursed of, consists in motion ; nothing else seeming to agree so well with the phænomena ? May not the sensory be considered as a substance composed of parts, influencing each other by mutual attractions, or otherwise ; and in course that different parts may be in different degrees of motion at the same time, according to the temperature of the parts of the body to which they answer ?

Obs. 6. The other senses must act with some force on the sensory, otherwise they cause no sensation. Thus, a bell vibrates a long time after
the

the sound ceases to be heard, as may be seen by throwing sand on it; and these vibrations doubtless affect the sensory, by means of the organs of hearing, with proportionable force. But the action not being sufficiently powerful, no sensation is caused. So bodies emit a considerable quantity of rays of light before they become visible. And the like may be observed of the other senses.

Obs. 7. I have had frequent occasion to observe that a force of action of the senses which at one time would cause sensation, at another time would not be sufficient for that purpose; and that the same force of their action would, at different times, cause sensations of different degrees of strength. When a person is weak, and feeble, he is much more susceptible of sensation than when in perfect health and vigour. But the following experiment will more fully illustrate this remark.

I have been for some time in a room strongly illuminated, and can behold the light of it without inconvenience. I now suddenly remove to an obscure one, where others who were here before, can see perfectly well. But the light is not sufficient to give me any sensation. After
 remaining,

remaining here some time I begin to discern the objects around me. The darkness continues to abate ; and I at length see as well as those who were here before. I again remove from this room to the former ; but the light of it is so strong that it quite dazzles me.— At length however I begin to view it with much less inconvenience, and it is now as indifferent to me as before.

By the first of these observations does it not seem probable that there is a force in the sensory which must be overcome before sensation can be caused ? By the second, that this force is variable ? That the sentient principle has a power or faculty of accommodating this force as much as it can to the action of the senses, so as to soften the strength of the sensation ? And that it can do this less, and also less speedily, as the action of the senses is less powerful ; analogous to what was observed with regard to heat and cold ?

I am aware that part of the foregoing effect will be imputed to the contraction or dilatation of the pupillæ of the eyes† ; and also to the optic nerves being partly rendered paralytic, agreeable to observations in my former essay on this sense ; and that in respect to heat and

† The pupil, on the contrary, is most dilated when the light is least.

and cold, part of the effect is to be ascribed to changes in the body, which will easily occur to the physiological reader. But, in both cases, is not a sufficient quantity of the effect to be attributed to the causes assigned ?

Obs. 8. In dreams, the ideas appear to us exactly the same as the sensations which we perceive when awake. Thus ; when we dream of a man, an house, the sky, or the like, we have the same perception of them, as of the originals when we behold them in waking. But while we are awake, the ideas are not thus perceivable.

The ideas (may it not be presumed ?) render themselves perceptible to us by the same means, or on the same principles, as the sensations do in waking. For like effects have like causes. Colours, and sounds, for example, are caused by vibrations ; and in order to render the effects of those vibrations sensible, they must overcome the force supposed to exist in the sentient principle ? And do the ideas become sensible by any other means ? As the ideas become perceptible during sleep, and cease to be so when we awake, and as we have no reason to conclude that the ideas act less forcibly in waking than in sleep, on the contrary,

contrary, analogy would argue that their action is stronger, does it not seem probable that the force in the sensory, already spoken of, is less in sleep than in waking ?

Obs. 9. In waking, the internal sounds, and colours discoursed of in my essay on the senses, are not usually perceivable. But if a person becomes faint, they begin to be perceived ; and become stronger according as the languor increases.

We have no reason to suppose that their action was stronger during the faintness than before : On the contrary, it would seem (by what will be found in the sequel) that they became weaker ; and as it appeared probable that there is a force, which they must have overcome in the sensory, before they could have become sensible, does it not seem to follow that this force was less during the languor, than before it ?

Obs. 10. From some of the preceding remarks it seems probable that when the body is languid, the force in the sensory is less than when it is in its full vigour. That this force becomes still less when the body is more faint ; and that in sleep it is least of all. Thus also we may ob-

E

serve,

serve, that when, by disease or otherwise, the body is weakened, the force in the sensory is less than in health. A state of the atmosphere which relaxes the body, weakens also the force in the sensory; as doth a contrary state thereof invigorate them. The like may be observed of sudden relaxations from revulsions: of hypochondriacal, and other similar affections of the body; and their contrary states. And many other instances will occur to the physiological reader.

Does it not appear probable therefore that there is a connection between the state of the body, and this force in the sensory? and that they have a mutual dependence or reciprocal influence on each other? If that be the case, what particular part or function of the body is it with which this supposed force corresponds?

It may be noted, that the functions of the body may be divided into *vital*, or *involuntary*; and *voluntary*, or *those with which the intelligent principle is concerned*. The former go on continually; in sleep, as well as in waking; and perhaps nearly alike at all times, if not even most in sleep. But the force in the sensory is less during this state of the body, than in waking; and in deep sleep it is probably little, or nothing,

as may in the sequel be attempted to be shewn. It would therefore seem obvious, at first view, that it is not with this part of the body that the imagined connection obtains.

But the latter functions are either wholly, or very nearly, inactive in deep sleep. With *this part* therefore the force spoken of would seem to be connected. And that this is the case would appear still more probable by a review of what has been said. For according as these functions of the body were more vigorous, the force in the sensory seemed to be greater. Now supposing this to be the truth, the following observation would point out to us the very manner, or form, of the connection in question.

Obs. 11. When a person is fainting, the internal sounds and colours become sensible, as hath been described. Yet, when he is quite fainted away, they *vanish*. But the ideas become as perceptible as real external sensations in waking. As he revives, the ideas disappear; but the noise in the ears, and colours before the eyes, return as strongly as before; but become less and less sensible till he recovers, when they also vanish.

These things hold good likewise with regard to sleep and waking ; but in this case the transitions are usually so swift, that the phænomena are seldom observable.

Let it be presumed, as before, that the action neither of the senses, nor ideas, was encreased in the above instances ; but only the force in the sensory so much lessened as to become inferior to their action. What I would remark in this case is, that as the person fainted, the internal sounds and colours became stronger ; yet when he was quite gone, and when therefore they ought, by reason of the diminished force in the sensory, to have become strongest of all, they disappeared. It cannot be said that they were overpowered by the ideas which then became sensible, because they acted more strongly than the ideas, as seems evident by their becoming sensible before them, and continuing after they disappeared, and therefore they ought rather to have obscured the ideas, than the ideas them. In fact, they absolutely disappeared during the syncope, and returned again when the person began to recover. During the syncope therefore, the senses were totally inactive

tive; but recovered their faculties as the fit went off?

When therefore the force in the sensory becomes very little, are not the senses deprived of their power or faculty of causing sensations? When that force is great, as in perfect health, the senses are in their full power of causing sensations when acted on by external causes; allowance being made for the force they have then to overcome in the brain. But when that force is very little, as in sleep, or a syncope, those faculties cease; the eye causeth no colours, nor the ear sounds, tho' strongly affected by the rays of light, or aerial vibrations: and even that usually insensible action of them which, during their active state, is continual in some degree, (as appears by the 9 and 11 observations,) wholly ceases in sleep, and fainting, as hath been seen. But at these times, it is well known that the parts of the body in question are in a *relaxed state*. In order to the due performance of their faculties they require to be properly actuated, or *braced up*. But this is an effect of *muscular power*; and therefore must depend on the action of the fibres, and the nerves serving for their *motion*. But to this state of the senses a certain

tain degree of the *force in the sensory*, discoursed of, seemed necessary. So that from this reasoning it would follow that the *connection is between the force in the sensory, and the muscular fibres of the voluntary parts of the body ; by the mediation of the nerves serving for the motion of these fibres.*

That this is the case seems also probable from the before mentioned property of the sensory, of accommodating its force as much as possible to the strength of the sensation. For if the sense be strongly acted on, not only the innate sounds, colours, or &c. will be proportionably excited, but the moving fibres of the part will, by the stimulus, have their action encreased. And as these will likewise augment the force in that portion of the sensory with which they correspond, the strength of the sensation, will, by the foregoing theory, be proportionally diminished. A variety of phænomena in the preceding observations, also render it *probable*, that the connection is between the sensory, and the *moving fibres of the voluntary parts of the body ;* as will, I imagine, be too obvious, to the philosophical reader, to need pointing out.

During

During the time of waking therefore, there seems to be a continual and reciprocal action and reaction between the moving fibres of the *voluntary parts of the body*, and the *sentient principle*, by the mediation of the nerves serving for the motion of the former. While this reciprocal action continues in due force, the body, and mind, are in states capable of properly acting on each other; the senses of the former on the mind, and the ideas of the latter on the moving powers of the body. When this reciprocal action ceases, the power of the senses to affect the sensory, and of the mind or ideas to move the body, likewise ceases; and in proportion as the former is affected, so is also the latter? If the *nature* of the connection, or reciprocal action spoken of, be asked, I can only answer, in the same diffident stile which I have all along observed in this enquiry, that *it seems to me to consist in VIBRATION.*

REMARKS

R E M A R K S

O N T H E

Foregoing Observations.

The only difficulty of moment, I imagine, that will occur to the reader in regard to the probability of the conjectures advanced, will be in what relates to the distinction of the motions of the body into voluntary, and involuntary. The involuntary, or as they should rather be termed, the vital, or natural motions, are those on which the circulation, and secretions, &c. more immediately depend, and are by the foregoing theory, independent of the sentient principle; or at least have no direct connection with it. For even in deep sleep these motions go on as well as in waking; and perhaps even better. The voluntary motions, are those which continue only in waking; either wholly ceasing in sleep, or at least being in great measure abated; and which from what has been said, appear to have a connection with

with the sensory, being influenced by the different states thereof. By this definition it will be seen that by voluntary motions, I do not mean those merely which are performed directly by the will, but that I include all those which depend on the sentient principle, or on the mind. Thus, the passions are not properly speaking, voluntary, but yet depend on the mind; and are found to influence the heart, and other moving, tho' involuntary, parts of the body. Voluntary motion, in the vulgar acceptation of the word, is confined to those motions which are performed by muscles having antagonists. We should therefore perhaps avoid confusion by distinguishing the two kinds of motions by other terms; suppose *continual*, and *occasional*? the vital motions, being understood by the former term; those depending on the sensory by the latter.

That these motions are essentially different, notwithstanding the contrary opinion of some ingenious writers, seems probable not only by the reasons which suggested the above terms, but also from the remarkable difference observed between them in the first part of this essay; where it seems to appear that heat is generated by the *continual* motions,

F

but

but not by the *occasional*. It seems necessary likewise that the nerves serving for occasional motion should all meet in a particular part, (the *corpus callosum* ?) Whereas nerves for the purposes of motion are known to originate from cortical substance, running through the whole length of the spinal marrow.

The heart is a muscle serving chiefly for *continual* motion, yet is also affected by *occasional* ; as is evident from the phænomena of the passions. It has therefore muscular fibres serving for both motions.

The voluntary muscles are also continually exerting involuntary action, and therefore likewise contain muscular fibres of both kinds ; but with this difference, that the proportion of fibres for occasional motion predominates in the voluntary muscles.

Tho' these motions are essentially different, yet they have a very observable effect on each other. If the fibres serving for *continual* motion are through any cause relaxed, the *occasional* motions are performed with more difficulty, than when they

they are in a contrary state : but then the reason is sufficiently obvious. Hence tho' the continual motions have no *immediate* connection with the sensory, or mind, they would seem by these effects (which yet are only *mediate*,) to have such connection.

The difference between these motions may further be collected from the following fact. I have several times had occasion to observe, that when, after fatigue, I had fallen into deep sleep in a sitting posture, and been suddenly awakened from thence, by violent knocking at the door, or other cause, my heart at first laboured with great and throbbing pain, and difficulty, to circulate the blood ; which however soon abated, and its action in a short time went on in the same imperceptible manner as usual.

During the sleep, the senses, and voluntary parts of the body (which make a great proportion of it) were in a relaxed state, and therefore the blood had a much easier passage through them than in waking, so that the muscular fibres serving for continual motion were sufficient for its circulation. But when afterwards the blood, from a

contrary state of the voluntary parts, came to be circulated with greater difficulty, these were not sufficient, but required the assistance of the occasional moving fibres. But by reason that I was so suddenly awakened, the latter, or rather, that part of them which served the circulating powers, had not, perhaps, sufficient time given them to return to their offices, and thence the painful labour of the heart?

Perhaps the action of the fibres serving for occasional motion, and their influence on the sensory, never wholly ceases even in sleep; at least this may be the case with those fibres which serve the more inward and vital parts of the body †; for our dreams often depend on affections of those parts in sleep. And it may already have been seen that the reciprocal action between these parts and the sensory is probably greater than at the surface. Hence also sleep invades the external parts sooner than the internal.

The medium between the fibre, and the sensory, was conjectured to be the nerve serving for the

† Those occasionally moving fibres which serve the circulating powers of the body, may therefore be considered as more active on the sensory; these serving the senses and direct voluntary parts, as more passive to it? See Observation II.

the motion of the former. As the nerve is in a more vigorous state, (*contains more phlogiston* ?) the reciprocal action will be greater; and contrariwise. On these circumstances the phænomena of waking, and sleeping; vigour, and weariness, may depend. But sleep and waking are also very powerfully influenced by *stimuli*, internal and external, as thinking, moving, uneasiness of body, light, sounds, heat, cold, &c. By means of these a waking state may be preserved tho' the nerves are far from being replete; whereas by removing them, sleep may be induced, tho' the nerves are in a vigorous state. The nerves of this class seem to be chiefly replenished in sleep?

It may appear at first view that the sentient principle derives its sentient faculty from its *vibration*. But on the contrary it may be noted that its sensibility is less, as the force of that vibration is greater.

It may also be imagined that heat and cold are contrary sensations. That heat is caused by increasing, and cold by diminishing the force in the sensory, which force may therefore depend on this sense. But the least reflection will shew that there can be no such thing as a negative sensation. Heat and cold seem to be only modifications of feeling. A body colder than the sense contracts the part, and by straitening the fibres serving for the sense of feeling, occasions that particular sensation?

sation? But a body that is warmer, puts them into a pleasing undulation? If however the heat be excessive, the fibres are lacerated by the expanded fluids, and hence pain arises. By excess of cold, a like laceration probably takes place, thro' the over contraction of the fibres, and the resistance of the fluid to them, especially if congealed.

As the most irritable parts are not always the most sensible, but in some cases the contrary, the moving fibres themselves (I mean the occasional ones) are probably insensible? Tho' they act ever so powerfully on the sensory, they may not cause *sensation*. That power may depend on a different mechanism. And are there not a set, or rather sets of fibres and nerves, distinct from those, properly adapted to these purposes, but which however require to be put into an active state by the moving fibres, agreeable to what has already been observed?†

It is less easy to reason plausibly on points purely *physiological*, than on *metaphysical* ones; and therefore, tho' I have not exhausted my fund, I shall not detain the reader any longer on the former subject. In respect to the latter, it may easily be conjectured that I have carried my ideas to a much greater length than in the foregoing observations. The philosopher will be able, from what has appeared, not only to anticipate me, but to

† Hence, by tying the artery, a muscle looses its *sense*, as well as motion. See page 10.

to push his enquiries perhaps much further than I have done. If metaphysical subjects were the taste of the age, I might be tempted to publish my speculations of that kind on a future occasion. As I believe, they are not, I shall hence forward suffer the remainder of my papers on those subjects, to rest quietly in my box, where they have lain (together with those which compose this second part) undisturbed, till now, for these twelve years past.

Since the first part of this Essay was printed the following has occurred to me.

1. If the surface of the crassamentum be covered with bile, it will not be reddened by the air. And if bile be mixed with the serum, the same effect takes place; at least in part. The like appearances in the blood of a jaundiced patient suggested these experiments. Hence tho' it should be denied that the bile, when *combined* with the chyle, tends to encrease the attractive force of the blood to be made from that chyle, yet that

he

the bile, when merely mixed with the blood, impairs its power of receiving and parting with phlogiston is evident: and, together with the languid state of the body in the jaundice, seems to prove the proposition, *that the action of the moving fibres is connected with the phlogification of the blood.*

I likewise found the venal blood in a cold paralytic limb to be of a more florid colour than usual; if I did not deceive myself.

It does not seem necessary; that the attracting particles should be red. For the flesh of chicken, &c. has little or no red blood in it.

2. Do not the capacities of those parts of the body, furnishing phlogiston to the blood *on which heat depends*, (see page 20) encrease with the heat, and diminish with the cold; of the external air? For cold is generated when the heat of the atmosphere is above 98, and heat when it is less.

3. Whether the red colour of the blood as well as its attraction for phlogiston does not depend on the bile combined with the chyle in the intestines? If blood be mixed with spirit of nitre there is only a change

a change from red to yellow ; but if bile be mixed with the same spirit the colour changes to blue red and yellow. As these changes depend on the loss of phlogiston, it would seem that, by the combination mentioned, the basis of the bile has its attraction for phlogiston only *lessened*.

4. Has not the gastric juice a contrary tendency to the bile in regard to the blood's attractive force ? And is not one use of the bile to counteract that tendency ? Also, do not acidities, in some cases, proceed from the gastric juice, which acidities are likewise obviated by the bile ?

5. Do not some of the sedatives and stimuli, (as they are called,) act by diminishing or increasing the blood's attractive force, and the action of the fibres in consequence thereof ? And do not many diseases arise from that attraction being too great, or too little ?

6. Whether the attractive force of the particles of the blood is not apt to vary, according to the food, and other circumstances ? If it be too near to that of the respired medium the blood will not be so easily decomposed in the organ of respiration ; and contrariwise. The colour of [the venal and
arterial

arterial blood, and the action of the fibres will in course be affected accordingly.

7. It may not be improper to add that the hypotheses of the vibration of the sentient principle seems to discover to us the reason why different times of vibration should cause different sensations.

On this principle, in conjunction with two or three others, the phenomena of sensation in general seem to depend, as far at least, as I have yet examined them.

The moving fibres may be put into vibration chiefly by the pulse of the blood.

Erratum. In page 29 line 19, for *left* read *more*.

For an account of the elastic power in animals see Mr. J. Hunter's Lectures.

12 MR 58

F I N I S.

